To remit, or not to remit: that is the question. A remittance field experiment

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To remit, or not to remit: that is the question. A remittance field experiment.

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Abstract

We conduct a remittance field experiment among Salvadoran migrants in the metro DC area. Migrants need to decide whether or not to remit funds to a recipient in El Salvador and if so how much. We maintain a (2x2) design in which the remittance budget has a value of $400 or $200, and the remitted funds arrive as cash or grocery vouchers that are non-transferable and applicable to basic necessities that do not include alcohol and cigarettes. Each migrant is randomly allocated to one of the resulting four treatments. We test across these treatments whether control over remittance spending in the form of grocery vouchers affects remittance behavior. We find the following. Our quantitative findings suggest that migrants prefer a remittance to arrive as cash than as groceries when stakes are high. This result is robust to inclusion of a wide set of covariates and is consistent with a conceptual framework in which migrants have preferences over how recipients spend remittances. Our qualitative findings suggest that migrants integrate amounts sent in the experiment with the external environment for sending remittances. We explore the mechanisms underlying the main effect and find that migrants who more recently sent a remittance and, in certain specifications, male migrants exhibit a greater preference for cash. Some implications of our findings are discussed.

Keywords: remittance, field experiment, remittance spending, El Salvador
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1. Introduction

Remittance flows form an important source of income and development for many developing countries. The World Bank (2011) estimates that $325 billion in remittances went to developing countries in 2010, representing close to 75% of total remittance flows. While the impacts of remittances seem to be relatively well understood and there appears to be consensus that remittances promote development (see for example Edwards and Ureta, 2003; Yang, 2006; Yang and Choi, 2007; Yang, 2008, 2011, and the references within), the exact drivers of remittance behavior are still being assessed.

The theoretical and empirical literatures on remittance behavior have posited and tested several hypotheses with regard to migrants' motives to remit, including altruism (Lucas and Stark, 1985; Agarwal and Horowitz, 2002), repayment of investment (for example in education, Johnson and Whitelaw, 1974; Faini, 2007), provision of insurance against shocks (de la Brière et al., 2002; Yang and Choi, 2007) and other forms of self interest driven by a search for bequests or esteem if returning home (Hoddinott, 1994; de la Brière et al., 2002; Yang, 2008). Recently, Ashraf et al. (2011), Chin et al. (2011) and Yang (2011) have indicated that the migrant's ability to control the use of remitted funds can also be seen as an important driver of remittance decision making.

In this paper we study two related questions: *Does full (relative to no) control over remittance spending affect the amount remitted from a fixed budget and does this effect vary with the size of the budget?* We conduct a remittance field experiment among Salvadoran migrants in the metropolitan DC area. We maintain a (2x2) experimental design in which the migrant's budget available for remittances has a value of $400 (high stakes) or $200 (low stakes) and the remitted funds arrive as grocery vouchers or cash. The grocery vouchers are by name of the remittance recipient, non-transferrable and only usable for basic necessities that do not include alcohol and cigarettes. Thus, they give the migrant full control over how the remitted funds are spent, but in only one dimension, *groceries*. In our experiment, a migrant is randomly allocated to one of the following four treatments: treatment 1, ($400,groceries); treatment 2, ($200,groceries); treatment 3, ($400,cash); and treatment 4, ($200,cash). This enables us to test across treatments 1 and 3 (2 and 4) whether full control over remittance spending on groceries, *specifically, the migrant's ability to control spending on alcohol or cigarettes*, affects the amount remitted.

The ex ante rationale for controlling remittance spending through basic necessities partly stems from evidence suggested by other data which show that migrants have a preference for spending on 'meaningful' items. For example, a survey conducted in 2008 and 2009 on behalf of the Government of El Salvador by IFPRI and FUSADES for evaluating the program Comunidades Solidarias Rurales suggests that migrants desire close to 75% of the remittance budget to be spent on household expenses (de Brauw, 2011).[3] The rationale for excluding alcohol and cigarettes from the coverage of the grocery vouchers is based on anecdotal evidence that migrants dislike remittances being put to unproductive uses, most notably, to purchase sin goods such as alcohol and cigarettes. This seems to be a broader concern in the household expenditure literature particularly when the person spending the money is male, as reported by for example Hoddinott and Haddad (1995) for the Ivory Coast, Ashraf (2009) (and the references within) for the Philippines, and International Organization for Migration (2010) for Moldova.

Our quantitative findings suggest that migrants prefer a remittance to arrive as cash than as groceries when stakes are high ($400), but not when they are low ($200). This result is robust to inclusion of a wide set of covariates (such as the migrant's and the recipient's gender as well as the migrant's risk, time, and social preferences) and is consistent with a conceptual framework in which migrants have preferences over how recipients spend remittances.

Our qualitative findings suggest that migrants integrate amounts sent in the experiment with the external environment for sending remittances. Approximately 80% of migrants report that they base the amount sent in the experiment on their own financial need (relative to the recipient's) and their typical cycle for sending remittances.

When controlling for migrants' qualitative reasons in the quantitative assessment, we find that migrants have an even greater preference for cash (over groceries) at high stakes. We explore the mechanisms underlying this effect and find that migrants who more recently sent a remittance and, in certain specifications,

[3] The other spending categories are education, health, business, construction, and any expense. The survey was not representative at the national level, but was mainly focused on rural areas in El Salvador.

male migrants exhibit a greater preference for cash. The former finding in particular is consistent with a framework in which migrants consider spending on groceries to be a basic necessity that should have priority over other types of spending.

Overall, two potential implications emerge from our results. First, it is important to be able to assess how subjects integrate decisions within an experiment with decisions in the naturally-occurring environment. Second, remittance products should give recipients sufficient spending flexibility. While control over spending may be desirable as suggested by previous literature (and possibly by the null effect at low stakes), migrants have a preference for cash at high stakes. As such, experimentation with remittance products that offer controlled liquidity may be a useful avenue for future research.

The remainder of the paper proceeds as follows. Section 2 explains the hypotheses, experimental design, and empirical strategy. Section 3 discusses the study implementation. Section 4 presents the main findings. Finally, Section 5 concludes and discusses some policy implications.

2. The experiments

Our experiments are designed to study two related questions: *Does full control over remittance spending on groceries (relative to no control) affect the amount remitted from a fixed budget and does this effect vary with the size of the budget?* An alternative way to frame the first question in our setup is to ask *whether the migrant's ability to control spending on goods and services other than groceries, in particular "sin goods" such as alcohol and cigarettes, affects the amount remitted.*

The rationale for these questions comes from an existing literature on intra-household allocations and spending (see for example Thomas, 1990, 1994; Strauss and Thomas, 1995; Udry, 1996, and the references within) that has specific applications in remittance contexts (see for example Lucas and Stark, 1985; de Laat, 2008; Yang, 2011, and the references within). The literature suggests that different actors within a household may have different preferences over household spending. In particular, in a migration context such as ours disagreements may arise between the migrant and the recipient regarding how remittances should be spent. As a result, the migrant's ability to control such spending may affect the amount remitted (see for example Ashraf et al., 2011).

2.1. Hypotheses and treatments

The economics literature has posited several models to explain the determinants of migrants' remittance decisions. Lucas and Stark (1985) present a model of tempered altruism or enlightened self-interest in which the migrant exhibits interdependent preferences and remittances are one element in a self-enforcing intra-household arrangement between the migrant and the recipient. Most models of remittance behavior go back to this rationale, viewing the migrant and the recipient as playing a (repeated) sequential game in which the household invests in the migrant in an attempt to spread risks across space and time. Remittances have thus been argued to be a form of insurance coverage or return on investment, apart from being driven by purely altruistic motives.

Appendix D formalizes a framework that leads to the following hypotheses:

H1: *Holding the budget size constant, the amount remitted can be different across spending on groceries (i.e. the grocery treatment) and spending on any goods (i.e. the cash treatment). The sign of this effect will depend on the migrant's perception of the recipient's level of spending on sin goods relative to all other spending, including groceries. If a migrant is willing to risk the recipient spending the remittance on sin goods, in order for her to have flexibility to buy items such as groceries, medicine, and clothing, she will send more in the cash treatment. On the other hand, if a migrant is concerned that the cash remittance will be spent on sin goods, she will send more in the grocery treatment.*

H2: *This effect can vary with the budget size. If the migrant perceives the high-stakes environment as more likely to give rise to overspending on groceries (sin goods) relative to all other items, she will remit less in the grocery (cash) treatment.*

Consistent with the aforementioned hypotheses, we varied two aspects across our experiment treatments: (1) the type of remittance use (across groceries and cash) and (2) the size of the budget available for remittances (across $400 and $200). This led to a (2x2) design comprising four treatments (see table

C.1): treatment 1, ($400,groceries); treatment 2, ($200,groceries); treatment 3, ($400,cash); and treatment 4, ($200,cash). We maintained a between-subjects design in which a given migrant was randomly allocated to one of these four treatments. The main details regarding the implementation of the treatments are discussed in Section 3.3. Here, we briefly discuss the rationale for the treatments.

In the cash treatments, the amount remitted arrived as cash. In the grocery treatments, the amount remitted arrived as grocery vouchers that were (1) in name of the remittance recipient, (2) non-transferrable and (3) not redeemable for alcohol or cigarettes. So, a comparison across the cash and grocery treatments enables a test of H1 (more below).[4] The grocery vouchers were for the supermarket chain 'Super Selectos' which according to a report by El Economista (August-September 2011) is the most widely preferred chain in El Salvador with a consumer preference rate of 60.6% (followed by La Despensa Familiar, La Despensa de Don Juan and Walmart at 17.9%, 11.9% and 3% respectively).[5]

The budget size (i.e. $400 or $200) was the maximum amount the migrant could remit within the experiment. Consistent with the conceptual framework, we wanted a high-stakes treatment that could potentially be beyond the satiation point. The American Community Survey (2008) suggested that Salvadoran migrants' monthly remittance amount was on average $288.18. So, we calibrated the low-stakes treatment ($200) at a slightly lower level and the high-stakes treatment ($400) at a higher level. These high stakes were also consistent with pre-experiment listing data which suggested that only 5% of our sample typically remitted above $400.

2.2. Empirical strategy

Given random treatment assignment, we primarily test H1 by running two types of regression equations:

- A basic specification

$$y_i = \beta_0 + \Sigma_{-j}\beta_{D_{-j}} D_{-j} + \epsilon_i, \tag{1}$$

  where y_i is the amount remitted by migrant i (this is our dependent variable *"sent_amount"*); β_0 is a constant; D_{-j} is a collection of treatment dummies excluding treatment j, which is taken as the baseline; and ϵ_i is an error term. At a budget size of $400, $j = 1$ such that treatment 1 (the $400 grocery treatment) is omitted. Thus, we are interested in the sign of β_{D_3}, the coefficient associated with treatment 3 (the $400 cash treatment). At a budget size of $200, $j = 2$ such that treatment 2 (the $200 grocery treatment) is omitted. We are interested in the sign of β_{D_4}, the coefficient associated with treatment 4 (the $200 cash treatment).

- An augmented specification

$$y_i = \beta_0 + \Sigma_{-j}\beta_{D_{-j}} D_{-j} + \Sigma_k \beta_{X_{ki}} X_{ki} + \Sigma_{-j}\Sigma_k \beta_{D_{-j}X_{ki}} D_{-j} X_{ki} + \nu_i, \tag{2}$$

  where X_{ki} represents a set of k proxies/covariates (we elaborate on these when discussing the data), $D_{-j}X_{ki}$ is a collection of interaction terms between the treatment dummies and the set of proxies, and all else is as defined previously. At a budget size of $400, treatment 1 is omitted and we are interested in the sign of $\beta_{D_3 X_k}$, which indicates whether the main treatment effect varies with the relevant set of proxies. At a budget size of $200, treatment 2 is omitted and we are interested in the sign of $\beta_{D_4 X_k}$.

A simple 'test' of H2 is obtained by comparing the main coefficients from regression equations 1 and 2 across stakes.

[4] While funds are fungible (in the sense that money not spent on groceries can be spent on other items), we note that ceteris paribus the grocery treatment gave the migrant better information how remittances would be spent, as one component of spending was more observable relative to the counterfactual cash treatment.

[5] Figure B.8 indicates the distribution of Super Selectos subsidiaries across Salvadoran municipalities at the time of our study. Figure B.9 overlays this distribution with that of the municipalities represented in our sample of remittance recipients. Section 3.1 will discuss how we arrived at this sample. As Section 4 will discuss, it is unlikely that our findings are driven by a lack of preference for grocery vouchers from Super Selectos. We find that migrants exhibit a strict preference for cash at high stakes, but not at low stakes. Such an effect is unlikely to be driven by an overall lack of preference for Super Selectos, since we would expect this to manifest itself uniformly across stakes.

3. Study implementation

It is estimated that approximately one out of seven to five Salvadorans live in the US and that they contribute anywhere from 8.59% to 16.94% of El Salvador's GDP annually in remittances (Terrazas, 2010; US Census, 2008; Central Intelligence Agency, 2010; Ratha et al., 2009). Therefore, the population of Salvadoran migrants in the US has been an ideal group for studying remittance behavior (some examples in the economics literature include Funkhouser, 1995; Edwards and Ureta, 2003; Ashraf et al., 2011).

A substantial proportion of the Salvadoran-Born population in the United States is concentrated in the Washington DC-Maryland-Virginia Metropolitan Statistical Area (US Census, 2008; Terrazas, 2010). Therefore, we selected the Salvadoran-Born population of the metropolitan DC area as our primary group of interest.

In what follows, we discuss (1) the sample; (2) the recruitment and listing of potential participants; (3) the experiment protocol; and (4) the implementation of the experiment sessions.

3.1. The sample

Our aim was to have a final sample of 200 migrants across the four treatments (i.e. 50 migrants per treatment). With an anticipated attrition rate of 50%, we aimed for a sample of 400 confirmed migrants. The recruitment steps discussed in the next section were geared towards having a list of at least 600 potentially participating migrants. 610 migrants were listed of whom 550 met the eligibility criteria for the study. 324 migrants confirmed their willingness to participate and were invited to attend a randomly assigned treatment (81 for treatment 1, 84 for treatment 2, 83 for treatment 3 and 76 for treatment 4).[6] Eventually, 133 migrants showed up for the experiments: 38 for treatment 1, 37 for treatment 2, 27 for treatment 3 and 31 for treatment 4.

Table C.2 compares the means of these basic characteristics across the different stages of sampling. The showed-up sample is not statistically different from the universe. An F-test for equality of means suggests that the samples are only different with regard to age at the 5% level. The table also includes averages for a nationally representative sample of Salvadorans across the US according to the 2008 American Community Survey (ACS, US Census, 2008). Our sample seems to be poorer and sending lower remittances than the nationally representative sample (these statements are not based on statistical tests), but this is not necessarily surprising given our sample mainly comprises blue collar migrants.

Table C.3 shows that there are no statistical differences in the means of basic characteristics collected at baseline (i.e. prior to the experiments) across treatments for the sample of migrants who confirmed and showed up at the experiments.[7] One exception is that significantly more women showed up for treatment 1 than for treatment 3. To minimize potential bias, we control for gender when comparing treatments 1 and 3.

Finally, we map the municipalities (municipios) of the Salvadoran recipients represented in our sample across El Salvador. Figure B.10 indicates that the Salvadoran recipients are quite spread out across El Salvador.

3.2. Listing

To gather the universe of Salvadorans in the metro DC area, we took several steps:

1. We hired three experienced recruiters who were trained extensively on (1) the importance of the study and how to pitch the study to potential participants (specifically, they were instructed how to explain the study without revealing too much information about its main question) and (2) the importance of respecting randomized treatment groups (i.e. to maintain the integrity of the study and achieve

[6] We also contacted the confirmed participants a second time (one day before the study). During both calls, participants were informed or reminded of the date, time and place of the study. They were asked to arrive 30 minutes prior to the beginning of the session and were informed that swapping sessions was not a possibility. All participants were reminded that, if they got to participate in the study, they would be paid a fixed amount of $40 and that there would be the possibility to win an additional prize. They were asked to bring picture identification as well as a sheet of paper with the name and contact information of the primary remittance recipient in El Salvador.

[7] Additional checks, in particular based on additional characteristics collected at endline, are available from the authors upon request.

proper identification). The recruiters were given IFPRI cellular phones to facilitate communication and IFPRI IDs and CASA de Maryland attire to exude more confidence.[8]

2. We reached agreements with several venues frequented by Salvadorans in the metro DC area (i.e., DC, Maryland and Virginia) in order to recruit on their premises. The main venues were (1) CASA de Maryland's three main workers' centers (Prince George, Wheaton and Langley Park), (2) the Salvadoran Embassy in DC, (3) CARECEN in DC and (4) Clínica del Pueblo in DC. All of these venues are known to cater to Central Americans in general and Salvadorans in particular. For a period of approximately four weeks, the recruiters visited these locations daily to recruit potential participants for the study. The typical flyer they used to recruit participants is indicated in figure A.7. Flyers were also left at the recruitment venues. In particular, CASA de Maryland posted the flyers on the doors of their main entrances. As indicated on the flyer, the criteria for participation in the study were (1) being Salvadoran, (2) being of a certain age (particularly, being 18 or older), (3) being able to read and write (i.e. having completed primary school), (4) typically sending remittances and (5) having some type of job (this did not have to be a full-time job).
   When migrants signed up for the study, they or the recruiter completed a form that contained the following information: name and contact information (i.e., phone, email and address), age, level of education, average monthly income, typical amount remitted, frequency and name of typical remittance recipient in El Salvador. This information served as a way to determine whether a migrant qualified as a potential participant. Furthermore, it would also enable us to assess any attrition bias.

3. Finally, one of the experimenters also made an appearance on Radio Zol, which is a popular radio station among Latinos (in particular, Salvadorans) in the metro DC area. During the broadcast, the study was explained and contact information was provided.

As previously mentioned, these efforts led to a total of 610 potentially participating migrants of whom 550 met all requirements for the study.

3.3. Experiment protocol

A main aim underlying our protocol was to reduce any distrust that might exist towards the remittance institution created as part of our experiment. While the budget available for remittances was provided by the experimenters and there were no fees associated with funds sent, ex ante we were still concerned that migrants may distrust the environment. In this section, we discuss in detail how we reduced this concern. Since all treatments were operationalized in the same manner, we first describe the basic premise of treatment 1 (the $400 grocery treatment). Then, we briefly discuss how the other treatments differed.

The migrant had to make a choice how much to remit from a budget of $400.[9] To facilitate the practical implementation of the experiment (in particular, payment), the migrant was restricted to remitting in increments of $50.[10] The migrant used the form in figure A.1 to decide how much to remit. The amount remitted would be paid in grocery vouchers to an indicated recipient in El Salvador no more than three days later. The amount the migrant decided to keep was paid at the end of the experiment (i.e., the same day) in the form of VISA gift cards. While these gift cards fulfilled the same purpose as cash in that they could

[8] CASA de Maryland is an NGO that was founded in 1985. It has a longstanding tradition of advocating for both migrant rights. It was created in response to the human needs of Central Americans arriving to the DC area after fleeing civil complexities in their home countries and thus, it is considered a "safe haven" for migrants. Our collaboration with CASA extended beyond just recruitment as will be discussed below; we also conducted the experiments at one of their locations.

[9] The remittance budget was provided by the experiments. As such, a migrant may view this budget as "windfall" and experience a so-called "house-money effect" (see for example Thaler and Johnson, 1990). We believe this is unlikely to confound our main treatment effects since (1) the experiments are conducted between subjects and (2) effects are identified across treatments of the *same* budget size. To the extent that migrants do experience windfall, we hypothesize that this effect would drive migrants to split the pie equally–that is, conform to the 50-50 norm–across the board. This effect would lead to an underestimate of the treatment effect.

[10] It is common to have discrete choice sets in lab-like field experiments of this type. The $50 increment was chosen to give enough variation in choices while still facilitating the implementation. Yang (2011) discusses that common remittance amounts for a sample of Salvadorans in the metro DC area are $100, $150, $200 and $300. So, a 'forced' choice set with increments of $50 seems reasonable given these migrants typical reality. Using the pre-experiment (recruitment) phase data, we find a similar pattern for the sample that showed up for our experiments (N=133); typical remittance amounts are $100 (24.81%), $150 (11.28%), $200 (25.56%), $250 (5.26%), $300 (12.78%), $350 (1.50%), $400 (4.51%), and $500 (4.51%).

be used at any establishment that accepts VISA, they made it practically impossible for migrants to divert *cash* outside of the experiments since they could not be returned or exchanged. As previously discussed, we wanted to mitigate this as much as possible, as it would undermine the purpose of the experiments, which was to elicit migrants' 'true' preferences for remittances. Payment through VISA cards did not fully eliminate the possibility for migrants to keep funds and purchase physical goods to be remitted outside the experiment. To get a sense of this, we asked migrants to explain the choices they made within the experiment immediately after making their decisions. Figure A.2 contains the exact framing of the question. A categorized version of the responses suggests that only 1.5% of migrants explicitly indicated that they were keeping funds for this purpose. We elaborate on these responses in Section 4.1.

The following measures were also taken:

1. The experiments were conducted at a location that was familiar and trusted by the migrants, particularly, those who might be illegal. We formed an alliance with CASA de Maryland to conduct the experiments at their Prince George's Workers' Center. This went hand-in-hand with recruiting participants at their locations as discussed previously.

2. CASA representatives were involved as much as possible in the implementation of the experiments. One of the main ways in which we did this was to have the manager of CASA's Prince George's Workers' Center hold a speech at the beginning of each treatment explaining primarily (1) what could be expected in the session and (2) that the experimenters were trustworthy partners of CASA. The speech, which was read aloud at the beginning of each session, is contained in figure A.3.

3. We gave a detailed explanation of (1) the decision the migrant faced (i.e. the choice set), (2) her potential earnings and those of the recipient and (3) how we would ensure that the remittance made it to the recipient.[11] This included a concise yet informative presentation of UNIMER Research International in El Salvador, the partner assisting us in delivering the remittances. An important component of this explanation was a pre-recorded video by a representative of UNIMER, which basically explained UNIMER's role in delivering the remittances to the recipients. The video also gave detailed information on the firm's contact information in El Salvador (i.e., address, telephone, email, and website) such that the migrant and/or the recipient could contact them as necessary. The video was shown prior to the migrants' decision making. The basic components of the UNIMER presentation and the text for the video are contained in figure A.4.[12]

4. Contrary to typical remittance carriers, migrants did not have to pay a fee for sending this remittance. This cost was born by the experimenters.

5. Each migrant who participated in the experiment received a phone card that would allow them to make a 10-minute call to El Salvador. Migrants could use this phone card to call the recipient in El Salvador to confirm whether the remittance had arrived. The experimenter handed out the phone cards *prior* to decision making to give a clear signal that migrants would be able to follow up on the remittance.

6. The migrant would be provided with a picture of the recipient in El Salvador as she received the remittance. UNIMER was instructed to take pictures of all recipients and send them to the experimenters. In turn, physical prints of these pictures were provided to CASA for pick-up by the migrants after the experiment.

7. Migrants were promised a fixed payment of $40 as compensation for (1) attending the session (i.e. to cover time, transportation etc.) and (2) completing the post-survey. We paid part of this ($25) upon arrival (i.e. immediately after migrants had been signed in and registered) to send a signal that decisions and earnings in the experiment would be for real. The remainder was paid upon completion of the study.

[11] The complete protocol is available from the authors upon request.

[12] The electronic file of the video can be obtained from the authors upon request.

Overall, these measures seem to have worked. 85.71% of migrants chose to send an amount greater than zero. Furthermore, responses to the open-ended question that followed the decision-making phase of the experiments suggest that migrants trusted the institution. For example, some migrants reported that they were sending lower (higher) amounts because of recent remittance sending (or lack thereof), which suggests that they saw our institution as an alternative avenue for remittance sending.

The other treatments followed the same protocol as treatment 1, except for the difference in remittance use and/or budget size. The remittance form for treatment 3 (the $400 cash treatment) is displayed in figure A.5. Similarly to the $400 treatments, the $200 treatments also required migrants to remit in increments of $50. So, the forms for the low-stakes treatments had a smaller set of alternatives. The forms are not included, but are available from the authors upon request.

Finally, since the budget sizes were calibrated around typical remittance amounts, budget limitations made it infeasible to have *all* migrant decisions become binding. So, we incorporated a lottery procedure into the payment protocol. In any given treatment, ten migrants would win the lottery and thus, have their earnings and those of their recipient become binding.[13] The exact procedures for the lottery implementation are discussed in the next section.

3.4. Experiment sessions

All experiments were conducted at CASA de Maryland's Prince George's Workers' Center. All treatments were conducted the same day to mitigate spill-overs. Each experiment treatment was randomly allocated to a time slot (session) during that day and participants who were randomly allocated to that treatment were invited for that specific session.[14] Participants knew that they were invited for a specific session, but did not know the details of that or other sessions such as the type of treatment.

The procedures for each session were as follows. Upon arrival, the identity of each participant was verified by matching the list of invitees for that session and their identification (either CASA ID, passport or driver's license). This was done outside in front of the main entrance to CASA's premises. Once verified, the participant was assigned a number and given access to CASA's premises.

The map to CASA's premises is displayed in figure A.6. It will assist in describing the different stages of each session and how we mitigated the possibility for participants to interact across treatments. Upon entry to CASA's premises, participants lined up in the corridor, as indicated by the arrow starting at point 0 in the figure. Each participant entered room 1 individually to complete the sign-in process. She showed the number that was assigned to her upon arrival to verify that she had passed the initial checkpoint. Then, she was asked for her ID again to confirm her identity. At that point she was assigned a unique study ID which would be used throughout the study to identify herself (as opposed to her name). This study ID was known only to her and was confidential towards other participants. The participant was then asked to provide the contact information (i.e., name, address, phone, email) of her respective remittance recipient in El Salvador (recall that participants were asked to bring this information when they were invited for the study). The participant then drew a number from an opaque bag. This number, which ranged from 1 to 50, was her seat number. After drawing the seat number, she was paid the $25 to partly compensate for attendance (recall the discussion in the previous section). She then entered room 2 and awaited additional instructions.

Room 2 was the main decision-making room. Upon entering room 2, the participant was instructed to find her seat number and locate herself at that station. She was then explained the consent form, which

[13] Since the number of lottery winners was fixed a priori at ten per treatment, as we were aiming for 50 participants per treatment, the probability of winning the lottery ended up varying across treatments. So, it could be argued that subjects who were in treatments that had a higher probability of winning were more likely to reveal their preferences for remittance sending. We believe this is of lesser concern since previous studies such as Laury (2006) have found that "lottery" and "pay-for-all" payment procedures elicit similar decisions. Furthermore, participants were urged several times prior to decisionmaking to take the decision seriously. To some extent, this was without reason since casual observation suggested that participants took the decisions quite seriously. This was further strengthened by the fact that the experiments were conducted during the earlier stages of the economic downturn.

[14] We randomized participants into pre-scheduled treatments/sessions as opposed to doing so upon arrival, since the latter would have complicated the implementation considerably. Namely, it would have required us to either have participants from different treatments in the same session and thus increase the probability of spill-overs across treatments or run simultaneous sessions and thus have multiple experimenters in different rooms. An alternative would have been to have participants choose how the remittance arrived (i.e. groceries versus cash) in addition to what was already being chosen. However, we wanted to ensure that a sufficient number of participants were faced with different treatment conditions (i.e. groceries versus cash).

she had to sign using her ID number to be able to participate in the experiment. Once all participants had signed the consent form, the session started. The CASA representative would hold his speech (recall figure A.3) and then the main experimenter, who was held fixed throughout all treatments, would explain the task at hand.

As mentioned previously, a complete set of protocols and instructions are available from the authors upon request, but briefly the procedure in room 2 consisted of: (1) an explanation of the remittance decision and of the lottery procedure for the decision becoming binding, (2) the presentation of UNIMER which included the video by the UNIMER representative, (3) distribution of the phone cards, (4) recording of the remittance decision, i.e., how much the migrant wanted to send (recall the record forms in figures A.1 and A.5) and (5) responding to the open-ended question which asked for the migrant's rationale for decision making.

Privacy of participants' decisions elicited in room 2 was an important component of our protocol, as we wanted to mitigate three types of "audience/social effects": (1) subject peer effects, (2) experimenter scrutiny effects and (3) recipient scrutiny effects. To mitigate issues associated with (1), participants were assigned IDs that were confidential towards other participants and participants made their decisions behind large voting boxes (comparable to dividers typically used in experimental laboratories). Also, when announcing the lottery winners (see the discussion related to room 5 below), these were announced using subjects' ID numbers.

To mitigate issues associated with (2), the experimenter retreated to a corner of the room and faced away from the participants while decisions were being made. Typically, the experimenter was the only person in room 2 with some exception being made in case some participants needed assistance completing the remittance form. Furthermore, subjects submitted their remittance record forms in closed envelopes that were marked with their seat numbers. These envelopes were collected by the experimenter and passed to assistant experimenters in room 3 (room 4 was used to keep extra supplies for the experiment such as additional forms). So, the main experimenter could not observe the decisions made in the session while it was in progress. To assure that migrants were correctly recording decisions, the assistant experimenters were in charge of (1) opening all envelopes, (2) verifying that the participant ID on the record form matched the seat number according to the sign-in sheet constructed in room 1 and (3) registering the decisions. Any mistakes were reported to the main experimenter who was in charge of addressing those with the participants. All windows between room 1 and other rooms where people may have been able to observe subjects' decisions–in particular room 3 and the corridor along point 0–were covered.

Finally, to mitigate issues associated with (3), the migrants were clearly informed that if they chose not to remit and won the lottery, we would not contact the "recipient" in El Salvador. This was motivated using parallelism with the external environment: On days when the migrant did not send a remittance, there was no reason for the recipient to go to the local branch of the remittance company or for the remittance company to contact the recipient. This was done to avoid that migrants felt obligated to remit (a type of "audience effect" explored by Andreoni and Bernheim, 2009, among others). It could very well be that this type of audience effect was minimal in our experimental context. This was likely to be the case if migrants considered the remittance decision within the experiment to be part of their day-to-day remittance cycle external to the experiment. While ex post evidence suggests that this was the case, when designing the experiment it was uncertain whether migrants would perceive the decision-making environment as such.

Once the participants completed the process in room 2 they proceeded to room 5. In room 5 they completed three tasks: (1) the post-survey, (2) the lottery and (3) the final payment. Those participants who were unable to complete the post-survey the same day were surveyed by one of the recruiters within one to two weeks after the experiment.[15]

The lottery was announced publicly using participants' confidential ID numbers to show that people from the session had actually won the lottery. The lottery was not announced by name to maintain privacy. Once the lottery was announced, participants were paid for their decisions. Participants received large thick envelopes that made it difficult to detect the envelopes' contents. This was done to protect the privacy of the lottery winners whose envelopes were more likely to be fuller due to the VISA cards. All participants were also paid the remaining payment for showing up ($15). Subjects were then free to leave. They were

[15] 11 participants did not complete the post-survey.

instructed to vacate the premises in order not to disturb the next session, which was already in room 1.

Each session lasted on average 150 minutes. All participants were paid a fixed amount of $40 as payment for showing up and participating. In addition, the 40 participants who won the lottery were also paid according to how much they had decided to keep. This amount was paid by means of VISA gift cards. On average those payments amounted to $148.75. So, overall, average payoffs were $84.74. This compares to an average daily income of $34.66 for the sample under consideration.

4. Results

To get a sense of migrants' sending behavior across groceries versus cash, we start with some basic statistics, tests, and graphs. On average migrants send $ 186.84 in the high-stakes grocery treatment and $ 222.22 in the high-stakes cash treatment. These amounts are significantly different at the 10% level based on a one-sided t-test. At low stakes, migrants send an average of $ 78.38 in the grocery treatment and $ 100 in the cash treatment. This difference is also significant at the 10% level based on a one-sided t-test. However, these are unconditional tests that do not control for observable differences across the treatments, as noted in Section 3.1 (more in Section 4.2 below).

Figure B.11 shows the empirical distribution functions of groceries and cash at high stakes. It appears that the distribution of cash first-order stochastically dominates the distribution of groceries. This suggests that overall (as well as on average) migrants send higher amounts in the cash treatment. A Kolmogorov-Smirnov full distribution test, however, cannot reject the null that these distributions are the same (p-value of 0.65).

Figure B.12 shows the empirical distribution functions of groceries and cash at low stakes. It appears that the distribution of groceries second-order stochastically dominates the distribution of cash. This suggests that on average migrants send similar amounts across these treatments, but that there is greater variability in the amount sent when it arrives as grocery vouchers. A Kolmogorov-Smirnov full distribution test rejects the null that these distributions are the same (p-value 0.097).

4.1. Qualitative insights

In order to gain a better understanding of migrants' decisions in the experiment, we asked them (after they had decided whether to send funds and the associated amount) to explain their reasoning. Figure A.2 contains the exact framing of the question. As first explained in Section 3.3, this was an open-ended question that migrants could answer however they wanted.[16]

In order to incorporate these responses into the analysis, we categorized them based on keywords and common rationales (a full report is available from the authors upon request). The first three categories are migrants who base the amount sent on their financial need relative to the recipient's. Category 1 comprises migrants who have *less financial need than the recipient*; category 2 comprises migrants who have *the same financial need as the recipient*; and category 3 comprises migrants who have *greater financial need than the recipient*. Category 4 comprises migrants who explicitly indicate a preference for remittances being spent on groceries. Category 5 comprises migrants who explicitly indicate a preference for remittances being spent on other items such as school fees, clothing, and medicine. Category 6 comprises migrants who explicitly cite their external remittance cycle, for example "I am sending this amount because I have not sent money in a while". Category 7 comprises migrants who perceive the experiment as a service being provided by a new remittance carrier. Category 8 comprises migrants who explicitly indicate that they are repaying the recipient. Category 9 comprises migrants who are concerned that the recipient resides far from a Superselectos branch. Finally, Category 10 comprises migrants who have other reasons for the amounts sent.

Table C.4 shows the distribution of categorized responses overall and by treatment. To confirm that these responses actually rationalize migrants' decisions in the experiment, we perform some checks. First, Category 4 is most likely to be reported in one of the grocery treatments since migrants in the cash treatments were not exposed to this option (recall the between-subject nature of our design). Indeed, the only cases where migrants indicate a preference for remittance spending on groceries are in treatment 1 – the

[16] One migrant did not answer this question.

$400 grocery treatment. Second, Category 9 should only be reported in one of the grocery treatments, since in the cash treatment the funds can be spent anywhere. Indeed, the only case is in treatment 2 – the $200 grocery treatment. Third, the only cases of Category 5, where migrants indicate a preference for spending on other items (such as school fees, clothing, and medicine) in addition to groceries, occur in treatments 1 and 2 – the grocery treatments. This makes sense, given these migrants are indicating that they like to control spending in more than one dimension – not just on groceries. Finally, if migrants truly weighed their financial need relative to the recipient's, as indicated by categories 1 through 3, we should observe different distributions of amounts sent across these three categories. In particular, migrants in category 1 should typically send more than half of the amount available to them (that is, the $400 or $200, depending on the treatment) to the recipient; migrants in category 2 should split equally; and, migrants in category 3 should send less than half. This is indeed the case: Of the migrants who fall in category 1, 80% sent at least 50% to the recipient; of the migrants who fall in category 2, 84% sent exactly 50% to the recipient; and finally, all migrants who fall in category 3 sent at most 50% to the recipient. So, overall the qualitative responses rationalize amounts sent in the experiment.

Having established that, we can now look at the main reasons for decisions made in the experiment. Table C.4 shows that 69% of the sample decide how much to send based on their own financial need relative to the recipient's (categories 1 through 3). Of the remaining, approximately half base the amount sent on their usual remittance cycle, that is, amounts sent outside of the experiment (category 6). These reports suggest that migrants "integrate" the remittance decision in the experiment with their day-to-day remittance behavior. Apart from giving external validity to the decisions made in the experiment, this suggests that we should control for migrants' responses to the qualitative question when conducting the quantitative analysis.

In what follows, we seek to test the main hypotheses discussed in Section 2.2 by primarily estimating regression equations 1 (Section 4.2) and 2 (Section 4.3). Given the alignment between the qualitative reports and the amounts sent, these specifications also control for migrants' reasons for remitting. To this end, we create dummy variables for the four most frequent categories (that is, categories 1, 2, 3, and 6) and include those as controls in the regressions. Finally, since the amount remitted is bounded below by zero and above by 400 by design of the experiment, our estimations follow a tobit specification (see discussions by de la Brière et al., 2002; Hoddinott, 1992, particularly in the context of remittance sending).

4.2. Quantitative findings

As discussed in Section 3.1, female migrants were significantly less likely to show up for treatment 3 ($400, cash) than treatment 1 ($400, groceries). In order to control for this observable difference across the two treatments, we include the migrant-gender dummy in all specifications in table C.5 ($400). For consistent comparison, the specifications in table C.6 ($200) also control for the migrant's gender (the significance of the treatment effect is no different if this control is omitted). All estimations pool the data across all treatments and control for three of the four treatment dummies, taking the omitted one as the baseline.

Column 1 of table C.5 and column 1 of table C.6 present the main estimates of regression equation 1 at stakes of $400 and $200 respectively. They suggest that migrants have a statistically significant preference for cash at high stakes (at the 10% level), but not at low stakes.

While these findings are consistent with the conceptual framework discussed in Appendix D, they do not account for migrants' qualitative responses. So, columns 2 through 8 in both tables control for the four main categories of qualitative responses (1, 2, 3, and 6) given by migrants (recall the dummy variables discussed in the previous section). Column 2 in particular shows that including these dummies strengthens the significance of the treatment effect at high stakes (table C.5). Migrants send significantly more in the high-stakes cash treatment at the 5% level (as opposed to the previous 10% level), once we control for their qualitative reasoning. Meanwhile, the effect remains insignificant at low stakes (compare columns 1 and 2 of table C.6).

Returning to the hypotheses in Section 2.1, the above findings are consistent with migrants being willing to sacrifice spending on sin goods in order for the recipient to have more flexibility when spending the remitted funds (this is the test of H1). This effect holds even after controlling for migrants' qualitative reasonings; in fact, it is stronger. However, the effect only holds at high stakes ($400). At low stakes, there is no significant difference between amounts sent in the grocery and cash treatments. This suggests that there is an effect of the budget size on migrants' perceived spending patterns (this is the test of H2).

4.3. Mechanisms and robustness checks

The framework developed in Appendix D suggests that differential behavior across the grocery and cash treatments is due to migrants' preferences for remittance spending. In order to explore this mechanism further and assess whether it is robust to alternative explanations, we expand the specifications in columns 1 and 2 of tables C.5 and C.6 by estimating equation 2.

One way to proxy for both the migrant's and the recipient's preferences for spending is to control for their respective genders. The use of gender as a proxy for preferences is supported by previous literature, which suggests that (1) there exist gender differences in preferences and decisionmaking (see for example Croson and Gneezy, 2009) and (2) women tend to be associated with higher (child) nutrition in the household (see for example Thomas, 1990, 1994).

Column 3 of both tables shows that including the migrant- and recipient-gender dummies does not alter the significance of the main effects. At high stakes, migrants still send significantly more when the remittance arrives as cash, although the point estimate is higher (127.50 relative to 50.37). Also, the interactions between the gender dummies and the main treatment dummy are not significant, suggesting that in this specification, men and women behave no differently across groceries and cash. At low stakes, all effects continue to be statistically insignificant.

In the remaining columns (4 through 8), we assess robustness of the findings in column 3 by adding different sets of covariates. Column 4 includes the following covariates to control for other dimensions of preferences: (1) risk (based on a hypothetical Binswanger, 1980, style lottery); (2) time (discount rates based on a hypothetical time preference question); (3) trust (based on the frequency with which the respondent lends money); and (4) altruism (based on a hypothetical dictator game). At high stakes (table C.5), the significance of the main treatment effect is unchanged, although the point estimate is lower (111.10 relative to 127.50). However, we note that both the migrant's gender and its interaction with the treatment dummy now become significant. This suggests that, after controlling for risk, time, and social preferences, male migrants (relative to women) send significantly lower amounts overall, but significantly higher amounts when stakes are high and the remittance arrives as cash. At low stakes, all effects continue to be statistically insignificant.

Column 5 of tables C.5 and C.6 expands the specifications in column 3 by adding two covariates to control for other remittance motives elicited through the post-survey: (1) a dummy for repaying one's family (for example for investments in education) and (2) a dummy for the recipient expecting remittances. The main effects are similar to those in column 4. At high stakes, the significance of the main treatment effect is unchanged. Furthermore, both the migrant's gender and its interaction with the treatment dummy are significant. At low stakes, all effects continue to be statistically insignificant.

The post-survey also collected stated measures that proxy for migrants' preferences for remittance spending. The first measure, grocery preference (gp), is a dummy that takes the value one if the migrant stated that she prefers remittances being spent on groceries versus other categories such as savings and investments (25% prefers grocery spending). Column 6 includes this dummy and its interaction with the main treatment effect. At high stakes, the effects become even more significant than in columns 4 and 5, while at low stakes, the effects remain insignificant. The second measure, normative spending (ns), is a dummy that takes the value one if the migrant stated that a budget of $400 *should fully be spent on groceries* (73% says it should be spent on groceries). Column 7 includes this dummy and its interaction with the main treatment effect. At high stakes, the main treatment effect is still significant at the 1% level, but migrant gender and its interaction with the treatment dummy are now insignificant. At low stakes, all effects continue to be statistically insignificant.

Finally, we assess whether migrants' decisions are responsive to a proxy for the remittance cycle external to the experiment. We construct a variable, last remittance (lr), that measures the number of weeks since the migrant last sent a remittance. As column 8 suggests, at high stakes, the main treatment effect is unaltered from the previous columns. In addition, the migrant's gender and its interaction with the main treatment dummy are significant as in columns 4-6. We also note that lr and its interaction with the treatment dummy are significant. These effects suggest that the longer it has been since the migrant sent a remittance, the more she sends overall and in the high-stakes grocery treatment. This is consistent with migrants seeing groceries as a basic necessity that needs to be fulfilled prior to any other spending. At low stakes, all effects continue to be statistically insignificant.

5. Conclusion

We conduct a remittance field experiment with Salvadoran migrants in the metropolitan DC area to test whether full (relative to no) control over remittance spending affects migrants' remittance behavior.

Our quantitative findings suggest that migrants prefer a remittance to arrive as cash than as groceries when stakes are high ($400), but not when they are low ($200). This result is robust to inclusion of a wide set of covariates (such as the migrant's and the recipient's gender as well as the migrant's risk, time, and social preferences) and is consistent with a conceptual framework in which migrants have preferences over how recipients spend remittances.

Our qualitative findings suggest that migrants integrate amounts sent in the experiment with the external environment for sending remittances. Approximately 80% of migrants report that they base the amount sent in the experiment on their own financial need (relative to the recipient's) and their typical cycle for sending remittances.

When controlling for migrants' qualitative reasons in the quantitative assessment, we find that migrants have an even greater preference for cash (over groceries) at high stakes. We explore the mechanisms underlying this effect and find that migrants who more recently sent a remittance and, in certain specifications, male migrants exhibit a greater preference for cash. The former finding in particular is consistent with a framework in which migrants consider spending on groceries to be a basic necessity that should have priority over other types of spending.

Overall, two potential implications emerge from our results. The first implication is methodological and indicates that it is important to be able to assess how subjects integrate decisions within an experiment with decisions in the naturally-occurring environment. Qualitative reports can be a complementary way to collect such data. The second implication is related to policy. Remittance products should give recipients sufficient spending flexibility. While control over spending may be desirable as suggested by previous literature (and possibly by the null effect at low stakes), migrants have a preference for cash at high stakes. As such, experimentation with remittance products that offer controlled liquidity may be a useful avenue for future research.

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Appendix A. Protocols and forms

IFPRI® UNIMER RESEARCH INTERNATIONAL CASA DE MARYLAND

Remittance Study
ID Number: _____

1. Would you like to send a remittance? Check "Yes" or "No".
   - Yes ____
   - No ____

2. If you do **NOT** want to send a remittance, choose option 1 in the following table.
   If you would like to send a remittance, choose one of the other options.
   You can only choose **ONE** option.
   The remittance amount you send will be used to buy household items (such as groceries) not including alcohol or cigarettes, by means of a certificate of the supermarket **SUPERSELECTOS**.

	I keep	I send	Check only ONE option
Option 1	$ 400	$ 0	
Option 2	$ 350	$ 50	
Option 3	$ 300	$ 100	
Option 4	$ 250	$ 150	
Option 5	$ 200	$ 200	
Option 6	$ 150	$ 250	
Option 7	$ 100	$ 300	
Option 8	$ 50	$ 350	
Option 9	$ 0	$ 400	

Figure A.1: Remittance form treatment 1 ($400,groceries)

Remittance Study
ID Number:_____

Question regarding the choice you made

In the space below, please explain why you chose to send (keep) the amount that you did, if you win the lottery at the end of the study
(The answer can be of any desired length)

_____

_____

_____

_____

_____

_____

_____

_____

Figure A.2: Question eliciting rationale for remittance decision

Welcome by CASA

Hello,

Thank you for coming to our study.

My name is _____ and I am _____ of this CASA Worker's Center.

I would like to take a brief moment to talk to you about this study, which is being realized by CASA de Maryland, IFPRI and Unimer.

In a few moments you will be given instructions, asked to make a decision and answer a few questions. CASA and I ask that you pay *very* close attention to all of these, especially the decision that you need to make.

Why?

First, this study can have important consequences for Salvadorians in this area and other Salvadorians in the US. So, it is extremely important that you pay close attention and make a serious decision.

Furthermore, during today's study you will make a remittance decision. **CASA and I guarantee that this decision is for real.** The exact decision will be explained in detail soon, but if you win a lottery at the end of today's study, **you AND your family** will be paid according to your remittance decision. So, please take this decision seriously, just as you do any remittance decision.

Finally, CASA and I would like to make very clear that this study is not about right or wrong decisions. What is important is that you make the decision that you most prefer. This is also why your decisions and the lottery winning are anonymous and confidential. **No one will know them.**

I realize that some of this may not make complete sense yet, but soon all of this will be clear. CASA just wants to take this time at the beginning of the study to reiterate to you its importance. The decision that you make is for real, so it is important that you think about it carefully.

Is this clear? If so, I will now hand it back to Angelino.
Again, thanks for agreeing to be in our study.

"Comment for speaker"
IMPORTANT: The speech is not about selling the idea of "sending remittances"! We do NOT want to bias the results. The speech is about inducing trust in the study. So, we need to be careful about how things are phrased! So, follow the script closely. Also, follow the script in all sessions. The endorsements MUST to be the same in all sessions.

Figure A.3: CASA representative speech

UNIMER Presentation

The UNIMER presentation contained the following elements:

1. A PowerPoint presentation of UNIMER discussing:
    a. Its objectives
    b. Its history (what it has accomplished etc.)
    c. Its staff (including pictures of the principle staff members)

2. A Video of UNIMER's main representative, which had as the main objective to explain UNIMER's role in delivering the remittances to recipients. This video is available from the authors upon request.

The speech of the UNIMER representative in the video was as follows:

Good day, I am _____, General Manager of UNIMER and I will be working together with IFPRI and CASA de Maryland to guarantee that the remittances you send to El Salvador reach your families. I am presenting myself, since my team and I will be in charge of assuring that if you are the winner of one of the lotteries and have chosen to send a remittance, this remittance will arrive according to your choice.

As the previous presentation has shown, UNIMER has been working in El Salvador and with the Salvadoran people for many years. As such we have vast experience on how to reach all parts of the country, as well as how to deal with the Salvadoran people with care and respect. Therefore, we would like you to know that if you win one of the lotteries and have chosen to send a remittance, you can rest assured that this remittance will be delivered to your family no later than this coming Tuesday or Wednesday. Together with IFPRI and CASA de Maryland, we will function in this case as a remittance company, with the only difference that we will not charge you for sending the remittance.

We would also like it to be clear that your decision to send or not is completely free of choice. If you decide not to send, your family will not be contacted and will not know that you chose not to send. We will only contact the families of those who win the lottery and decided to send a remittance. If there are people that choose not to send and they win, we will not speak to their families.

For you to have even further assurance that the remittance was delivered to your family, we invite you to call them Tuesday or Wednesday of this coming week and ask them if they have received it. To facilitate this call, Angelino will give you a long-distance phone card that you can use for this purpose. Furthermore, we will take a picture of your family receiving the remittance and we will send these to CASA de Maryland so you can come pick it up later. Finally, if you need to contact UNIMER, our contact information is the following: PHONE _____, ADDRESS _____, EMAIL _____ and WEBSITE _____.

Ok, I hope you are enjoying this study and that we can be of service to you.

If you have any questions with regard to UNIMER or the study, you should feel free to ask Angelino at this time.

I send warm greetings from El Salvador to my fellow countrymen.

Figure A.4: Components of UNIMER presentation (including text for pre-recorded video)

IFPRI®

UNIMER RESEARCH INTERNATIONAL

CASA DE MARYLAND

Remittance Study
ID Number: _____

1. Would you like to send a remittance? Check "Yes" or "No".
   - Yes ____
   - No ____

2. If you do **NOT** want to send a remittance, choose option 1 in the following table.
   If you would like to send a remittance, choose one of the other options.
   You can only choose **ONE** option.
   The remittance amount you send will be paid **IN CASH**.

	I keep	**I send**	**Check only ONE option**
Option 1	$ 400	$ 0	
Option 2	$ 350	$ 50	
Option 3	$ 300	$ 100	
Option 4	$ 250	$ 150	
Option 5	$ 200	$ 200	
Option 6	$ 150	$ 250	
Option 7	$ 100	$ 300	
Option 8	$ 50	$ 350	
Option 9	$ 0	$ 400	

Figure A.5: Remittance form treatment 3 ($400,cash)

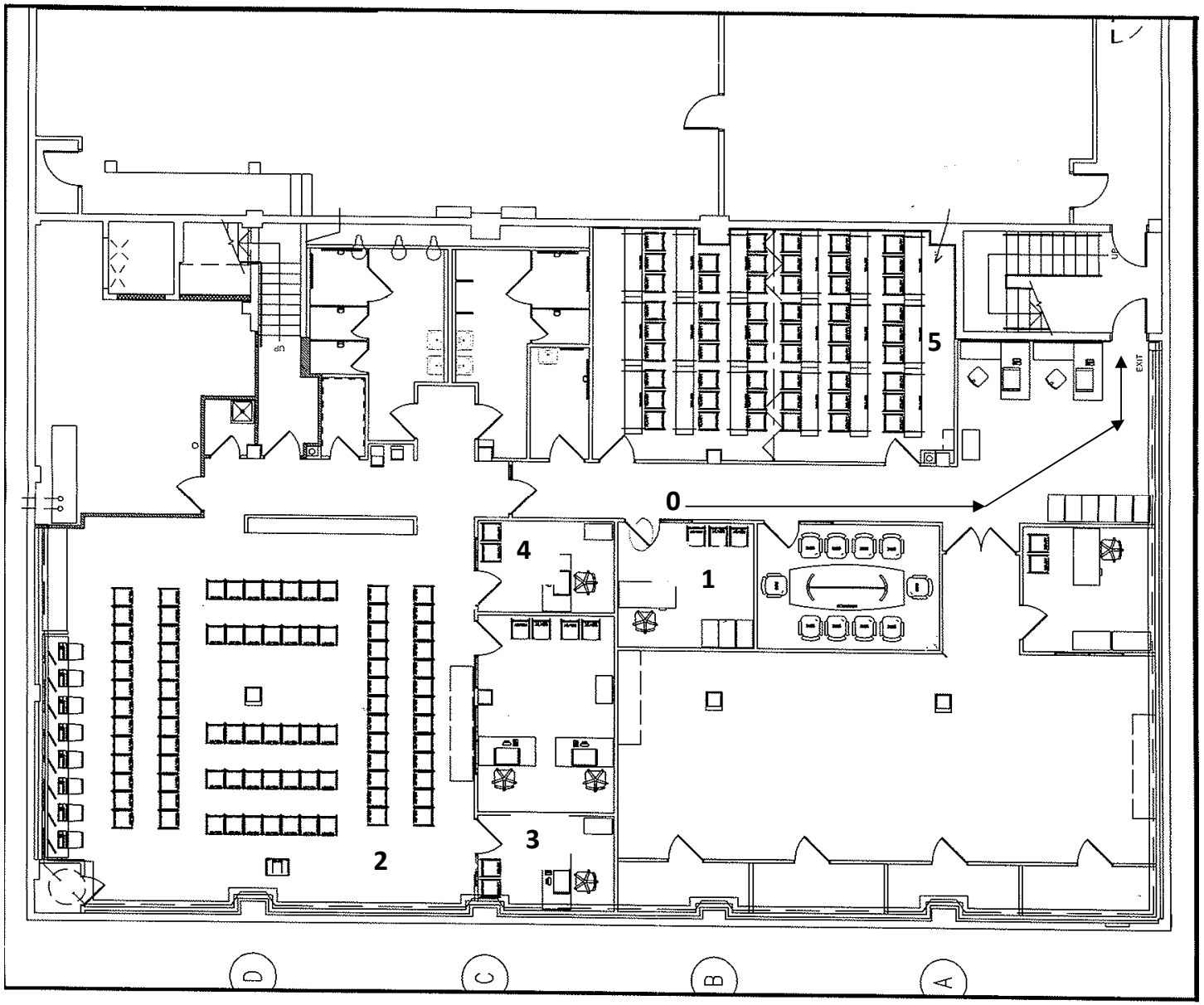

Figure A.6: Map of CASA's premises

Figure A.7: Typical recruitment flyer

Appendix B. Figures

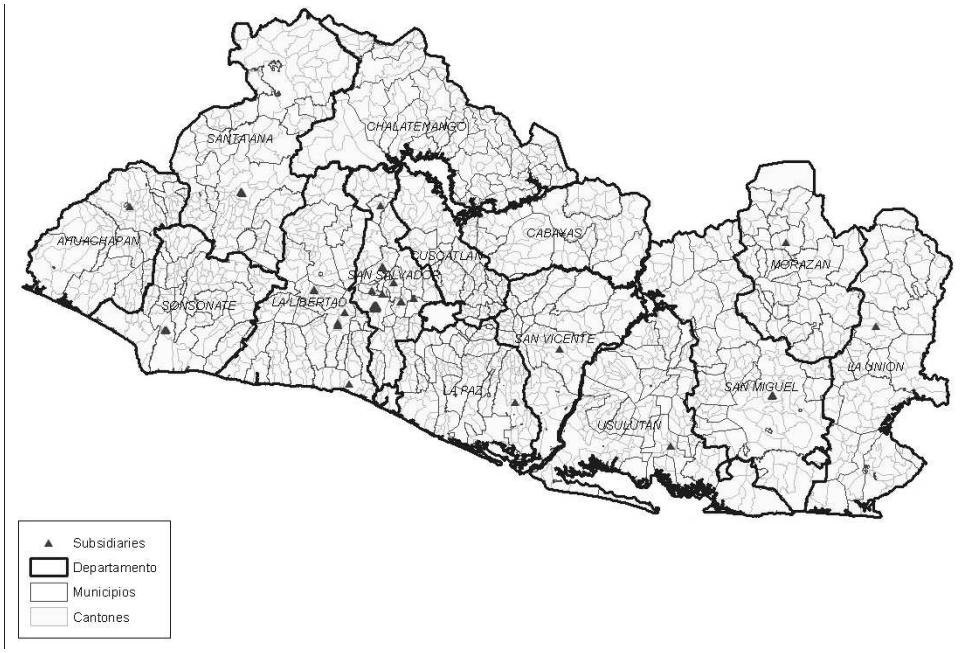

Figure B.8: Municipios (municipalities) with Super Selectos supermarkets

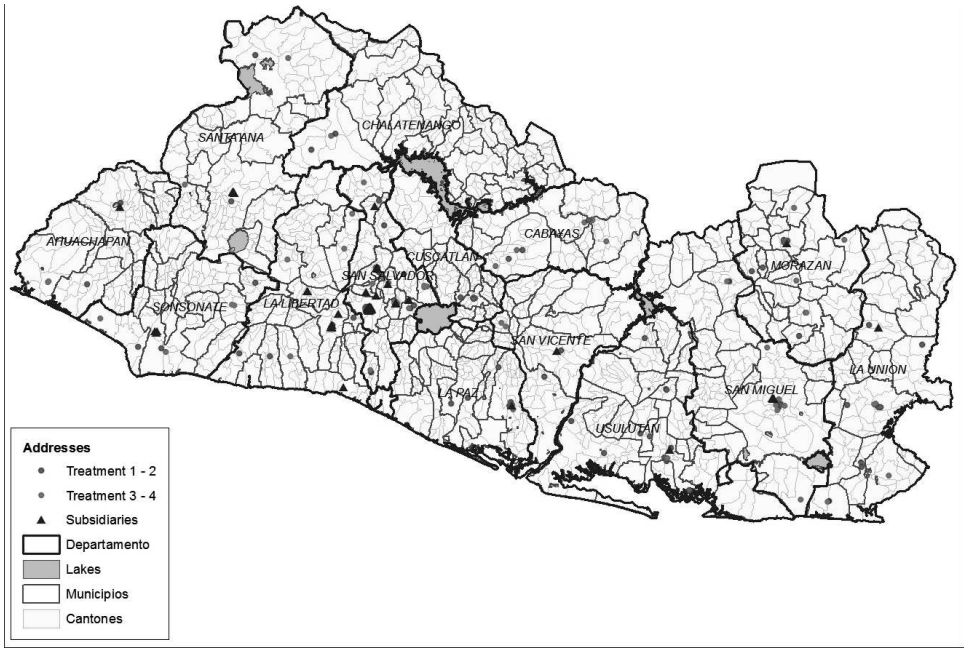

Figure B.9: Municipios (municipalities) with Super Selectos supermarkets and sample of recipients

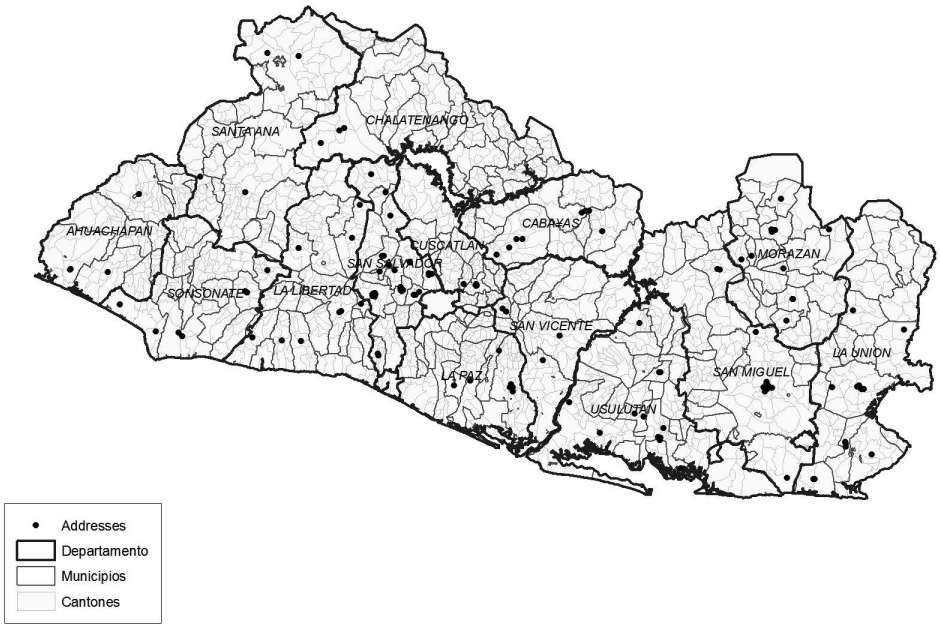

Figure B.10: Municipios (municipalities) with sample of recipients

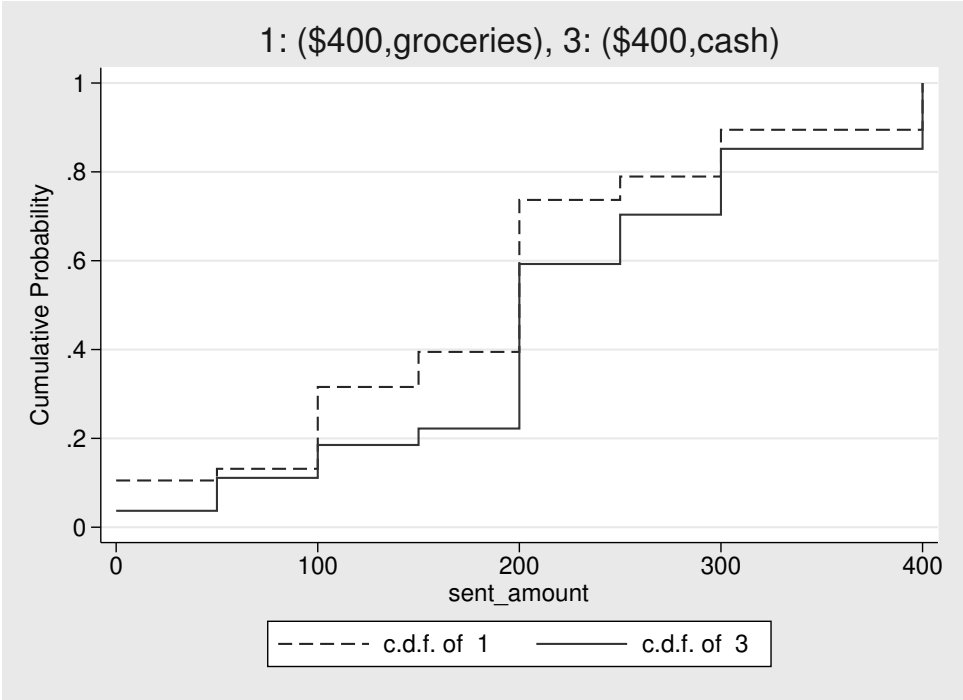

Figure B.11: Distributions of amount sent (groceries and cash at $400)

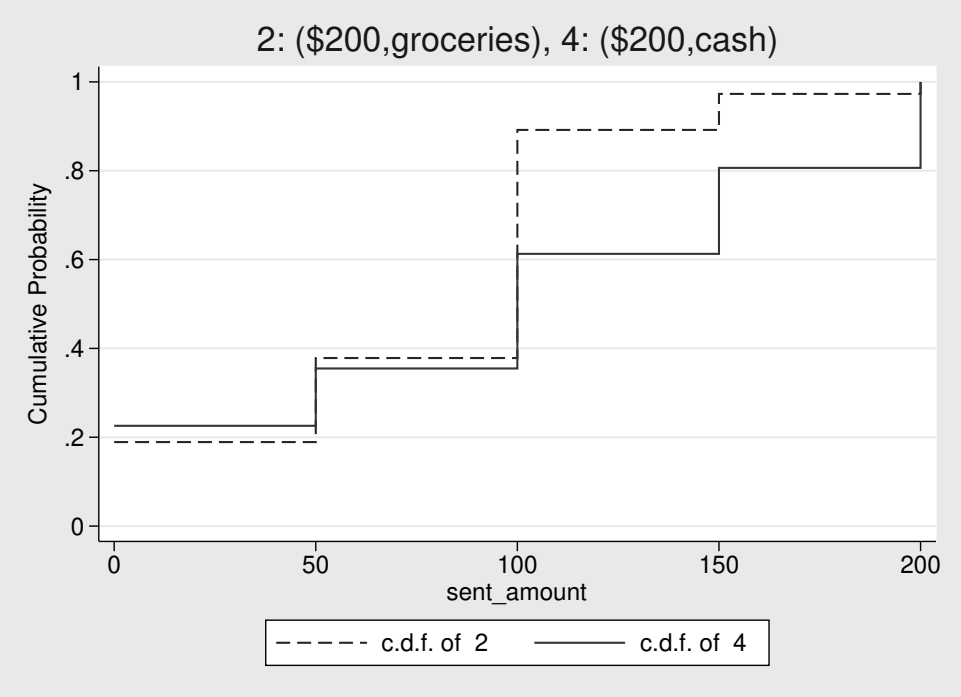

Figure B.12: Distributions of amount sent (groceries and cash at $200)

Appendix C. Tables

Table C.1: (2x2) Experimental design

		Remittance use	
		Groceries	Cash
Budget size	High: $400	1: ($400,groceries)	3: ($400,cash)
	Low: $200	2: ($200,groceries)	4: ($200,cash)

Table C.2: Means-comparisons of pre-treatment characteristics by sample[a,b]

	N	migrant gender	age**	recipient gender	remittance	income	primary school
Universe	550	0.371	36.493	0.810	267.727	1319.618	0.867
Confirmed	324	0.354	36.525	0.801	270.367	1326.491	0.860
Showed-up	133	0.391	38.797	0.782	278.316	1320.451	0.879
Surveyed	122	0.385	39.123	0.779	280.115	1316.148	0.884
ACS 2008	700,000	0.472	39.187	–	288.179	1838.583	–

[a] Based on F-tests for equality of means performed across all samples *excluding* the ACS sample.

[b] All variables as defined in table C.3.

** Age is significantly different at the 5% level; no other variables are significantly different.

Table C.3: Means-comparisons of pre-treatment characteristics by treatment[a]

	Sample	1: ($400,groc)	2: ($200,groc)	3: ($400,cash)	4: ($200,cash)	$\Delta 12$	$\Delta 34$	$\Delta 13$	$\Delta 24$
migrant gender	Confirmed	0.395[b]	0.415	0.277	0.329	-0.020[d]	-0.052	0.118	0.086
(1=female)		(0.492)[c]	(0.496)	(0.450)	(0.473)	(0.077)	(0.073)	(0.074)	(0.077)
	Showed-up	0.553	0.486	0.185	0.258	0.066	-0.073	**0.367*** **	0.228
		(0.504)	(0.507)	(0.396)	(0.445)	(0.117)	(0.111)	(0.116)	(0.117)
age	Confirmed	35.432	35.671	36.928	38.171	-0.239	-1.243	-1.496	-2.500
(in years)		(10.257)	(9.340)	(10.220)	(11.332)	(1.536)	(1.709)	(1.599)	(1.647)
	Showed-up	38.079	37.541	37.741	42.097	0.538	-4.356	0.338	-4.556
		(11.849)	(10.897)	(11.040)	(11.542)	(2.631)	(2.978)	(2.900)	(2.726)
recipient gender	Confirmed	0.802	0.756	0.843	0.803	0.046	0.041	-0.041	-0.047
(1=female)		(0.401)	(0.432)	(0.366)	(0.401)	(0.065)	(0.061)	(0.060)	(0.066)
	Showed-up	0.763	0.676	0.889	0.839	0.087	0.050	-0.126	-0.163
		(0.431)	(0.475)	(0.320)	(0.374)	(0.105)	(0.092)	(0.098)	(0.105)
monthly remittance[e]	Confirmed	256.765	269.500	280.060	275.211	-12.735	4.850	-23.295	-5.711
(in US$)		(150.340)	(179.088)	(157.508)	(377.869)	(25.915)	(45.232)	(24.054)	(46.504)
	Showed-up	266.184	295.757	282.407	268.807	-29.573	13.601	-16.223	26.950
		(166.138)	(222.265)	(137.850)	(201.795)	(45.231)	(46.076)	(39.036)	(51.912)
monthly income	Confirmed	1357.037	1285.244	1401.446	1256.579	71.793	144.867	-44.409	28.665
(in US$)		(570.941)	(605.127)	(798.206)	(676.823)	(92.174)	(117.914)	(108.599)	(102.000)
	Showed-up	1307.105	1352.703	1394.444	1233.871	-45.598	160.573	-87.339	118.832
		(634.737)	(663.947)	(749.786)	(592.085)	(149.964)	(176.354)	(172.305)	(153.955)
primary school	Confirmed	0.877	0.805	0.855	0.934	0.072	-0.079	0.021	**-0.129** **
(1=completed)		(0.331)	(0.399)	(0.354)	(0.377)	(0.057)	(0.058)	(0.054)	(0.062)
	Showed-up	0.842	0.838	0.926	1.000	0.004	-0.074	-0.084	-0.162
		(0.370)	(0.374)	(0.267)	(0.447)	(0.086)	(0.099)	(0.083)	(0.100)

[a] Based on t-tests for equality of means performed on pairwise treatments for each sample.

[b] mean for given treatment group, [c] standard error in parenthesis. [d] Δij represents the difference in means for treatment group i and j (i.e., mean$_i$-mean$_j$).

[e] Also tested but not included is the remittance frequency. The variable is not significant in any comparison.

*** p<0.01, ** p<0.05. Only differences that are significant at the 5% level (and below) are reported.

Table C.4: Distribution of reasons for amount remitted[†]

Category	Overall	1: ($400,groc)	2: ($200,groc)	3: ($400,cash)	4: ($200,cash)
1. Migrant has less need	50/132	13	10	12	15
2. Migrant has same need	19/132	9	5	3	2
3. Migrant has greater need	22/132	3	9	3	7
4. Spending on groceries	2/132	2	0	0	0
5. Spending on other items	2/132	1	1	0	0
6. Remittance cycle	19/132	6	1	7	5
7. New remittance service	4/132	0	3	1	0
8. Repaying recipient	1/132	0	0	1	0
9. No Superselectos branch	1/132	0	1	0	0
10. Other reasons	12/132	4	6	0	2

[†]One individual did not respond.

Table C.5: Preference for groceries or cash at $400?[†]

VARIABLES	(1) sent_amount	(2) sent_amount	(3) sent_amount	(4) sent_amount	(5) sent_amount	(6) sent_amount	(7) sent_amount	(8) sent_amount
cash 400	50.55*	50.37**	127.5**	111.1**	122.1**	156.0***	159.7***	152.8***
	(27.56)	(24.29)	(53.73)	(55.78)	(51.79)	(54.12)	(60.40)	(52.80)
migrant gender (mg)	29.45	26.23	55.63	187.6**	162.4*	228.9**	0.507	148.1*
	(19.63)	(17.19)	(65.73)	(91.54)	(87.23)	(90.88)	(63.76)	(86.80)
mg * cash 400			-127.7	-250.6**	-215.1**	-295.4***	-51.07	-220.0**
			(78.60)	(100.6)	(96.88)	(100.7)	(77.76)	(95.74)
recipient gender (rg)			15.39	-105.9	-73.05	-127.1	92.96*	-75.03
			(66.22)	(92.11)	(87.89)	(88.91)	(55.43)	(86.84)
rg * cash 400			-54.48	82.12	40.63	92.51	-114.4	45.82
			(83.33)	(109.1)	(101.3)	(101.9)	(73.39)	(100.1)
risk preference[a]				-3.799				
				(17.20)				
time preference[b]				27.74				
				(27.13)				
trust[c]				-5.249				
				(13.09)				
altruism (1=selfish)[d]				-15.03				
				(19.32)				
repayment motive					64.24			
					(42.06)			
expectations motive					28.07			
					(16.93)			
grocery preference (gp)						67.81*		
						(35.83)		
gp * cash 400						-74.37		
						(67.07)		
normative spending (ns)							-3.955	
							(34.70)	
ns * cash 400							-57.13	
							(54.94)	
last remittance (lr)								7.479**
								(3.612)
lr * cash 400								-8.275**
								(4.049)
Constant	170.4***	160.5***	132.9***	162.3***	123.0***	109.3***	147.3***	117.6***
	(20.35)	(24.68)	(28.73)	(47.36)	(29.89)	(33.61)	(36.27)	(30.97)
$\sigma^{\ddagger}$	105.0	90.27	86.69	83.70	82.65	82.26	82.39	81.60
	(7.660)	(6.499)	(6.236)	(6.218)	(6.136)	(6.111)	(6.220)	(6.046)
Observations	133	132	132	122	122	122	119	122
Log likelihood	-674.6	-652.6	-647.8	-600.4	-599.2	-598.7	-581.3	-597.1
Pseudo-R^2	0.0320	0.0610	0.0679	0.0702	0.0721	0.0728	0.0745	0.0754
Interactions[e]	N	Y	Y	Y	Y	Y	Y	Y
Qualitative[f]	N	Y	Y	Y	Y	Y	Y	Y

[†] Tobit estimates. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [‡] Analogous to the square root of the residual variance in an OLS regression.
Data pooled across all treatments. Groc 400 is the baseline and cash 400 is the comparison. The other treatment dummies are included in all columns but not reported.
[a] Based on a hypothetical Binswanger (1980) style lottery choice framework. [b] Based on choices over hypothetical amounts of money today versus in one month.
[c] Based on the frequency with which the migrant lends money when asked. [d] This is based on a hypothetical dichotomous choice dictator game.
[e] Whether interactions between the other treatment dummies and the respective covariates are included. [f] Whether qualitative dummies are included. N=no, Y=yes.

Table C.6: Preference for groceries or cash at $200?[†]

VARIABLES	(1) sent_amount	(2) sent_amount	(3) sent_amount	(4) sent_amount	(5) sent_amount	(6) sent_amount	(7) sent_amount	(8) sent_amount
cash 200	26.00	7.590	-51.88	-57.42	-67.10	-90.32	-9.865	-44.57
	(26.52)	(24.49)	(58.62)	(62.59)	(61.92)	(65.46)	(70.06)	(61.99)
migrant gender (mg)	29.45	26.23	23.30	11.84	20.46	12.88	11.35	11.67
	(19.63)	(17.20)	(30.18)	(30.44)	(30.19)	(29.74)	(30.27)	(29.52)
mg * cash 200			16.37	21.95	20.14	25.14	8.464	15.17
			(46.87)	(47.09)	(46.19)	(45.84)	(47.16)	(46.10)
recipient gender (rg)			17.66	31.88	14.88	25.14	23.40	24.86
			(31.44)	(32.65)	(31.99)	(31.20)	(32.13)	(30.86)
rg * cash 200			54.74	48.76	74.06	76.58	69.56	71.76
			(58.83)	(64.47)	(63.75)	(63.30)	(64.07)	(62.17)
risk preference[a]				-3.799				
				(17.20)				
time preference[b]				27.74				
				(27.13)				
trust[c]				-5.249				
				(13.09)				
altruism (1=selfish)[d]				-15.03				
				(19.32)				
repayment motive					64.24			
					(42.06)			
expectations motive					28.07			
					(16.93)			
grocery preference (gp)						-8.552		
						(33.54)		
gp * cash 200						55.34		
						(49.78)		
normative spending (ns)							16.26	
							(38.17)	
ns * cash 200							-73.31	
							(54.57)	
last remittance (lr)								1.670
								(1.606)
lr * cash 200								-4.158
								(2.987)
Constant	51.66**	67.91***	56.44*	82.51*	53.18	64.95*	57.88	57.46*
	(20.29)	(22.85)	(33.34)	(47.27)	(32.84)	(34.83)	(41.60)	(32.44)
σ[‡]	105.0	90.27	86.69	83.70	82.65	82.26	82.39	81.60
	(7.660)	(6.499)	(6.236)	(6.218)	(6.136)	(6.111)	(6.220)	(6.046)
Observations	133	132	132	122	122	122	119	122
Log likelihood	-674.6	-652.6	-647.8	-600.4	-599.2	-598.7	-581.3	-597.1
Pseudo-R^2	0.0320	0.0610	0.0679	0.0702	0.0721	0.0728	0.0745	0.0754
Interactions[e]	N	Y	Y	Y	Y	Y	Y	Y
Qualitative[f]	N	Y	Y	Y	Y	Y	Y	Y

[†] Tobit estimates. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [‡] Analogous to the square root of the residual variance in an OLS regression.
Data pooled across all treatments. Groc 200 is the baseline and cash 200 is the comparison. The other treatment dummies are included in all columns but not reported.
[a] Based on a hypothetical Binswanger (1980) style lottery choice framework.
[a] Based on a hypothetical Binswanger (1980) style lottery choice framework. [b] Based on choices over hypothetical amounts of money today versus in one month.
[c] Based on the frequency with which the migrant lends money when asked. [d] This is based on a hypothetical dichotomous choice dictator game.
[e] Whether interactions between the other treatment dummies and the respective covariates are included. [f] Whether qualitative dummies are included. N=no, Y=yes.

Appendix D. Conceptual framework

While the setup we develop below is not one of explicit imperfect information, it is sufficient to show how remittance spending by the recipient affects remittance sending by the migrant. Furthermore, by design of our experiments, the migrant in principle has perfect information how remittances are spent in the grocery treatment. Given this is sufficient ex ante to identify a differential effect across the treatments, we believe our conceptual framework provides enough insight into testable hypotheses. Models of explicit imperfect information that could be amenable to our setup are discussed by for example Ambler (2013) and Chen (2013).

Consider a *migrant* who (1) spends her wage income w on either a remittance ρ or general consumption $y = w - \rho$; (2) does not save or borrow; and (3) remits to only one *recipient* whose budget income is determined purely by the remittance.[17]

The migrant's utility function[18] is

$$u = g\left[y, v(c, z)\right], \tag{D.1}$$

where v is the recipient's utility, $z = \rho - c$ is the recipient's consumption of sin goods, and c is the recipient's consumption of all other goods (including groceries). In this formulation the migrant's utility is determined by (1) own consumption and (2) the recipient's utility, which depends on her consumption of sin goods and all other goods, which are in turn determined by the remittance level.

To get a sense how this formulation is useful for our purposes, consider the recipient's and the migrant's optimization problems.

The recipient takes the remittance as given (treating it as an income parameter) and chooses grocery (and other) consumption to solve the following problem

$$\max_{c} v(c, \rho - c), \tag{D.2}$$

which has the following first-order condition (FOC) implicitly defining Marshallian demand functions for consumption of all other goods $c^*(\rho)$ and sin goods $z^*(\rho)$[19]

$$v_c - v_z = 0.^{20} \tag{D.3}$$

The migrant then takes the recipient's Marshallian demand functions $c^*(\rho), z^*(\rho)$ as given and chooses the remittance to solve the problem

$$\max_{\rho} u = f[y, v(c^*(\rho), z^*(\rho))], \tag{D.4}$$

which has the following FOC implicitly defining an optimal remittance level ρ^*

$$F \equiv -f_y[y, v(c^*(\rho), z^*(\rho))] + v_c(c^*(\rho), z^*(\rho))(f_{v_c} c_\rho + f_{v_z} z_\rho), \tag{D.5}$$

where F is defined as the expression for the FOC. This expression makes use of the fact that $v_c = v_z$, which follows from the recipient's first-order condition (see D.3).

The migrant's FOC suggests that the optimal remittance level will primarily depend on the migrant's willingness to trade off own consumption for the recipient's utility (i.e. f_y, f_{v_c}, and f_{v_z}), and the recipient's preferences for sin and all other goods (v_z and v_c). This can be further assessed by using the implicit function theorem (IFT) to sign the following first-order partial derivative: $\partial\rho/\partial c^* = -(\partial F/\partial c^*/\partial F/\partial\rho)$. By the second-order condition of the migrant's optimization problem, we know that $\partial F/\partial\rho < 0$ if she is

[17] While simplistic, this latter assumption need not be unrealistic. For example, consider the case in which the migrant is remitting to an elderly parent or to children. Although the remittance may not be the sole income source, it may be large enough relative to other income sources that it can be modeled as such. In fact, data collected in an IFPRI survey conducted among 4494 rural households throughout the Northern part of El Salvador (mainly in the "cantones" of Chalatenango and San Miguel) show that remittances constitute 22.9% of household income–the same as income from agricultural production.

[18] We assume all utility functions are neoclassical (i.e. twice continuously differentiable and strictly concave).

[19] Throughout the analysis, we normalize all prices to one.

[20] We use subscripts attached to functions to denote first- and second-order partial derivatives. So v_k indicates the first-order partial derivative of v with respect to k, whereas v_{kj} indicates the second-order partial derivative of v with respect to k and j respectively.

to achieve a maximum. Accordingly, the sign of $\partial\rho/\partial c^*$ depends on the sign of $\partial F/\partial c^*$. In particular, $\partial\rho/\partial c^* \geq 0$ (i.e., the migrant's remittance is nondecreasing in the recipient's optimal consumption of all other goods) if $\partial F/\partial c^* \geq 0$. Similarly, $\partial\rho/\partial z^* \leq 0$ if $\partial F/\partial z^* \leq 0$.

Deriving the expression for $\partial F/\partial c^*$ we have

$$\partial F/\partial c^* = [f_{yv}v_c + (v_{cc} - v_{cz})]\,(f_{v_c}c_\rho + f_{v_z}z_\rho), \tag{D.6}$$

which has been simplified by making use of the recipient's FOC.

Following from the assumptions of neoclassical utility, we have that $v_c > 0$ and $v_{cc} < 0$. The sign of $\partial F/\partial c^*$ depends on:

1. Whether the migrant's utility is increasing or decreasing in the recipient's consumption of all other goods, i.e. the sign of f_{v_c}.
2. Whether the migrant's utility is increasing or decreasing in the recipient's consumption of sin goods, i.e. the sign of f_{v_z}.
3. Whether the marginal utility the recipient obtains from additional consumption of all other goods increases or decreases with additional consumption of sin goods, i.e. the sign of v_{cz}.

The above comparative statics suggest that whether the remittance is increasing or decreasing in the recipient's consumption of all other goods depends on how much she trades off the recipient's consumption of all other goods for sin goods. Without casting further assumptions, the prediction could go either way. However, it may be reasonable to assume that $f_{v_z} < 0$ and $f_{v_c} > 0$ such that the sign of D.6 will depend on the migrant's dispreference for sin goods vis-à-vis all other goods. So, we may expect the migrant to exhibit differential behavior across the grocery and the cash treatments.[21]

[21] See previous version of the paper for an alternative model that distinguishes between two goods: grocery consumption and all other goods including sin goods.